A CONTRIBUTION TO THE LIFE HISTORY OF THE DIATOMACEAE — PART II.

H. L. SMITH, Hon. F. R. M. S., Geneva, N. Y.

III. *Motion of the Diatoms.*

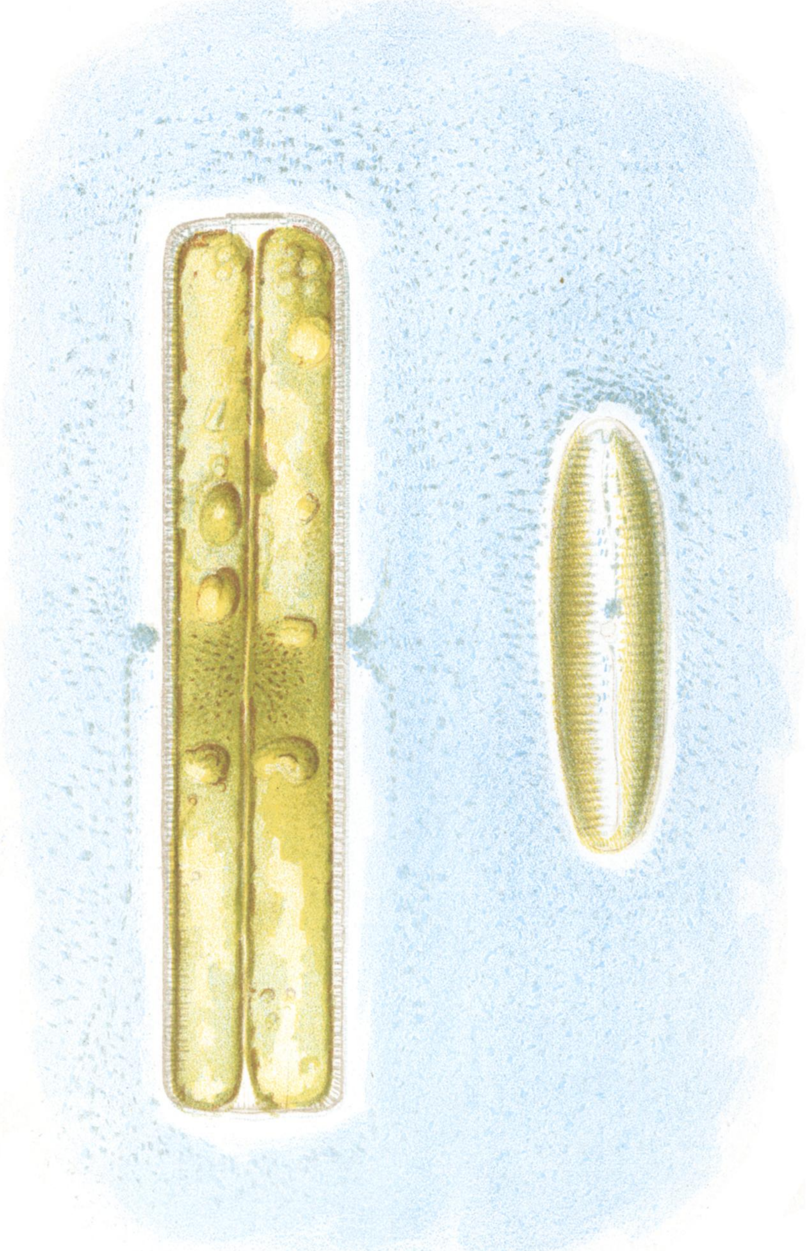
Various explanations have been offered to account for the movements of the Diatoms. Ehrenberg supposed that he had observed a sort of protuded foot, others have imagined they could perceive ciliæ, and some very good observers think they have seen an undulating protoplasmic sheath outside the frustule. None of these explanations are really satisfactory, and we may affirm without much fear of contradiction, that, up to the present time, the whole subject is involved in obscurity.

Without attempting a review, or critcism of the work of others, I shall confine myself here, simply to a statement of my own observations bearing upon this subject, presenting them for what they may be worth, and regretting that, upon the whole, they are not more satisfactory. In investigating the cause of the movements, it may be remarked, that in a general way, all diatoms having a raphé, like the Naviculæ for example, when they are moving, present the valve and raphé to some solid substance, gliding along, e. g., on the surface of the glass slide, or the covering glass, when we examine them with the microscope, and if by a slight movement, or some disturbance, they are rolled over, they quickly right themselves and are again presented in "side view" with the raphe towards the surface of the glass, or may be, rise up on one end and swing around. This fact is familiar to all observers of living diatoms, and because of this peculiar-

ity, they present themselves always, or almost always when moving, as I have said, in "side view," i. e. with the valve, not the sutural zone, towards the eye. The diatoms without a raphé, as the Nitzschiæ, which have very lively motions, and the Surirellæ which move more sluggishly, glide along with the ala, or keel, in contact with some foreign substance, or, if they themselves are not moving, little particles of matter can be seen running along the ala, or the keel; or the raphé, if the diatom belongs to group I. This movement of small particles in a contrary direction to that in which the diatom was moving, before it was stopped, can be very readily observed, and is too well established to be questioned. When one has watched a diatom for sometime, moving forward in a certain direction, and then perceives the path suddenly to be retraced, this appears to be so much like a voluntary action, or as if in search of food, that we do not wonder that many of the earlier observers, including Ehrenberg, regarded them as animals. Most of my own experiments have been made with large specimens of *Navicula* (*Pinnularia*) *major*, and it was in November, 1865, that I first detected, in observing this diatom, the very curious phenomenon that I shall now describe. Several times since, I have seen the same, but never more perfectly than when first discovered. I had made a pretty deeply colored solution of the ordinary water color pigment, indigo, which, while showing to the eye a dark blue tint, exhibited under the microscope numerous minute particles undissolved, and in fact, it was but the myriads of these particles out of focus, that gave the general tint to the field. I transferred to the indigo field some specimens of large living *Navicula major*, which were in a healthy condition and had been recently gathered; I perceived immediately something that gave the impression of a transparent slime, or mucus sheath, outside these diatoms; that this appearance was due to some transparent investing mucus or jelly-like substance, and not an optical illusion, was manifest from the gathering together of the particles, in a lunate manner in

front of the moving diatom, as they were pushed forward and by their opening behind. This appearance is represented around the smaller of the two diatoms figured in Plate I, and I could also see as represented in the plate, the little undissolved particles, running along the median line, or raphé, towards the central nodule, here they stopped for awhile and then finally passed out focus. Although the gathered particles, pushed in front of the moving diatom, seemed to indicate beyond a doubt the presence of an external transparent sheath, thinner perhaps along the median line or perhaps perforated, I was not satisfied, and hastened to examine other diatoms, I soon found that there was a very great difference in the amount of this external slime, some diatoms showing it abundantly, and others very sparingly, and indeed, the same diatoms, sometimes exhibited more and sometimes less of it; and as a general rule, those with the darkest endochrome showed the least of this external substance. To test still further whether the appearance was an illusion, I put large frustules of the prepared diatoms, and also some dead forms, but still having more or less endochrome in them in a similar field; these presented no such appearance as that I had observed with the living specimens, though in the case of some of the dead forms, still having the endochrome in them, I could perceive traces of it. So strongly apparent is this investing sheath, that it can be seen excellently with as low a power as a two-thirds inch objective, if the diatoms are large and in a healthy condition. To remove all doubt, I put into the same field, two large specimens of the same diatom, and of equal size, one quite active, the other dead; the investing substance was quite evident around the former, but not around the latter. Again, I warmed a slide with the Naviculæ in the indigo field, but not so as to be uncomfortably hot to the hand; the diatoms were killed and the endochrome changed in color to green, the frustules appeared now to be inside of almost colorless sacs surrounded by the indigo, and tinted only by the particles of indigo out of focus above and

PLATE I



below them. I rolled the frustules over and over repeatedly and across the field; the investing substance, whatever it was appeared to be something insoluble in water, though, as I found out afterwards, and as I have already mentioned in Part I. it is readily tinted by fuchsine, and, in fact, it seems to be, but in much smaller quantity, the same substance that is found around diatoms in the act of conjugating, and which is in this case, readily recognized as it is more or less stained or dirty, as well as more abundant. A large specimen of Stauroneis, did not exhibit a trace of this mucus. But again it was quite distinct surrounding many desmids, (e. g. Euastrum), and when these latter were moved about they left a trail behind.

Not only do the little particles glide along the raphé of a Navicula, as I have described, but there is something more wonderful, and it was quite startling to me when I first observed it, as I now seemed to be getting some clue to the cause of the movements of the diatom. I have endeavored to represent the phenomenon to which I allude in the larger figure, Plate I., and the diatom is here presented in front view; it had been stopped in its movement by some foreign body, not shown in the drawing.

I saw the little particles of indigo, streaming along on either side, by a series of jerks, towards the central nodule. When they arrived at the end of the raphé, just where, on the prepared valves of the large Navicula, a distinct point, or dot, can be seen at the end of the raphé, near the central nodule, they stopped, and were here gathered into *rotating balls*, which, after acquiring a certain size, suddenly broke, and the individual particles streamed off as shown in the drawing in a direction opposite to that in which the frustule had been moving before it was stopped. I noticed this curious phenomenon for hours, and saw ball after ball thus formed and dispersed, and have since shown it to others. Sometimes, the motion would stop for an instant, and then commence suddenly in a reversed direction, the particles now running

along the raphé on the other half of the valves and the diatom itself commenced to move in a reversed direction. In other words, when the diatom tended to move in any direction, the particles would move in the opposite one; but when the diatom was moving freely, the little particles were quiet, it was only when it was stopped, or delayed by some obstacle, or about reversing its motion that the particles began to move, and form the balls that I have mentioned. In plate I., the particles are shown some distance from the valves, but I have, in other cases, noticed them almost or quite in contact, and, in any case, it would seem as though there must be an opening or slit in the mucus sheath, or perhaps the elevated ridge of the raphé, or ala, extends through it, and it would appear also that a true hole or opening in the valve existed at the "dot," at the end of the raphé, near the central nodule, and that here at least, there was more or less of an opening through the investing sheath. I have frequently observed (in front view) a difference in thickness of the sheath on the two valves, and sometimes a movement of the particles on one side, and not on the other, in which case the diatom turns around or even lifts itself up. The rotation of the little balls of indigo, which is the most curious part of these observations, there can be no doubt about, and we may well ask, how is it to be explained? One can readily fancy a minute jet of the fluid, issuing from the interior of the frustules, through the little opening, or dot, and thus causing this rotation, it is not unlike the well known hydraulic experiment which I have frequently exhibited of a cork ring clinging to the side of a jet of water and rotating; but if this be so, where is the water entering the frustule to keep up the supply? Is it possible that, on the "central band," already mentioned in Part I., as a fulcrum, the valves of the frustules alternately open and close at either end, thus causing an ingestion on one side, and an ejection at the same time of the previously ingested fluid at the other? And, if this be so, what is the cause of this motion, from which, if it exists, that of the diatom itself may result?

It is not difficult to conceive that if osmotic action is going on, and one can not doubt that this is the case, a difference of tension may exist in the two halves of the frustule, on either side of the middle band; and this would produce a bellows-like movement of the valves, and give rise to the issuing jets on the one or the other side. I set myself therefore to work to determine, if possible, whether any such motion of the valves, as here supposed, does in reality exist. The observations were made with difficulty, as the diatom must not only be large and quite lively, but it must be arrested in its movements if micrometrical measurements are to be made, and yet, itself, so far as the valves are concerned, exhibit the movements, if any really exist, which, under ordinary circumstances, would have resulted in motion of the diatom.

Selecting a large specimen of *Navicula major* which was imbedded so firmly in a slime mass that it did not move, and which was presented in front view, I carefully adjusted the spider lines of an excellent Powell & Leland micrometer on the over-lapping edges of the hoops, or zones, I watched this specimen for half an hour, and observed, during this time, a movement of increase and decrease, and the whole extent of the variation in breadth, in the opening and the shutting of the edges of the hoops, and presumably the movement of the valves, amounted to between 10 and 15 divisions of the micrometer head, or, with the power I was using, 1 50.000th of an inch. Subsequently I repeated the experiment on a very large *Stauroneis Baileyi*, one end of which is shown in Fig. 1.

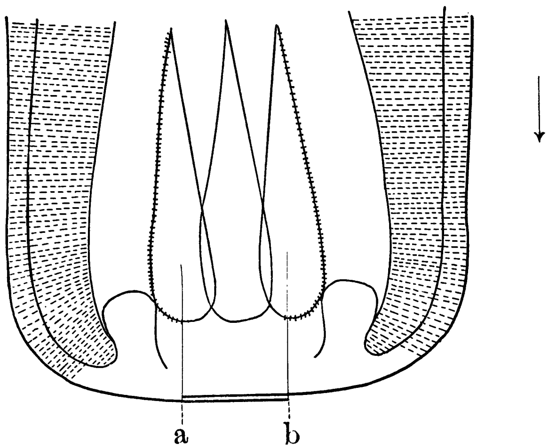


Fig. 1.

This diatom was partly free, and could occasionally move a short distance among the débris in which it was imprisoned, and I was enabled to make tolerably satisfactory measurements. When the movement of the frustule was in the direction of the arrow I found, in one case, a change of distance occurred between *a* and *b* equal to 19 divisions of the micrometer head, and shortly after, an increase of 4 divisions more, and I satisfied myself that the frustule opened slightly in the forward moving end; I am unable to say whether, at the same time, it closed an equal amount at the other end. I need not remark that, at first, this was just the opposite of what I had expected; for I had expected to find that, in moving forward, the anterior portion of the frustule would close, inasmuch as the issuing jet which had caused the rotation of the indigo balls came from this portion; the explanation of this anomaly is not difficult. I have already mentioned that the internal contents of the diatoms are enclosed in a membraneous sac, which is, no doubt, elastic, and at all times under tension. If we suppose endosmose in one half, and exosmose in the other, there would be a swelling of the sac enclosing the internal contents on one

side and a diminution on the other, attended by an opening of the valves in the swelling half; this would be attended also with an expulsion of the water lying between the membrane and the under surface of the siliceous valve on the same side, and a reception of it on the other — though probably, now not along the raphé and through the dot, but in the opening line of suture, as the particles of indigo are never to be observed moving along the raphé of the following end. Doubtless the water containing the carbonic acid, and whatever else there may be in solution capable of being assimilated, bathes the membraneous sac in between it and the inner surface of the siliceous valves, and the nutritive material finds entrance into the sac by endosmose, alternately at the opposite halves, the expelled water being that from which the nutriment has been taken, more or less completely. I am disposed, therefore, to consider that the motion of the Naviculæ is due to injection and expulsion of water, and that these currents are caused by different tension of the membraneous sac in the two halves of the frustule. In those diatoms which do not have the central band thus binding the frustules, as, for example, the Synedræ, the Fragilariæ, and the circular and angular forms, no motion is to be observed, or at best but a slight trembling, as, in this case, the tension is more nearly uniform over the whole surface of the internal sac.

In the representation of one end of Stauroneis, given in Fig. 1, markings may be seen on the internal zone, corresponding to the large thickened infolding of the valves, known as *vittæ* in the smaller forms, just within where the internal zone is connected with the valve. These markings indicate successive points of growth of the internal zone, or connecting membrane, which increases in breadth, probably not by a deposition of silex on the outside edges, but at the junction with the valves, and the smaller amount of silex here, or the less perfectly siliceous character compared with the other portions of the frustule, is indicated by the slight adherence

of the zones to the valves, in all except the filamentous species, noticeable especially after treatment, and also by these zones remaining often projecting over the new frustule, after self-division, as in Melosira, and Biddulphia. To return to the movement of the indigo particles, one can observe this to occur with great rapidity along the ala of the Surirellæ and the keel of the Nitzschiæ, and the motions of the latter are especially rapid. In these two genera the particles run along the whole length of the ala or keel, but generally only on one side, though I have seen them in the case of Surirella moving up one side and down the other at the same time, producing now the "languid roll." So far as the Nitzschiæ are concerned, and which have the most lively motions of any diatoms, the structure appears to be that of a Navicula without central nodule, and so, having a complete (not divided) raphé, which runs now, not somewhat near the middle of the valve as in the Naviculæ, but along one edge, directly in contact with the sutural zone, it is along this that the little particles can be seen to move with great rapidity when a large Nitzschia has its motion stopped by meeting some opposing obstacle, but I have never witnessed the formation of the balls, as in the case of *Navicula major*, nor have I ever detected any of these movements in the frustules of Group III. As for the presence of ciliæ, I have, hitherto, been unable to detect them, using various methods of illumination, and the very best objectives; and I have, for this purpose, watched for hours the changes in the endochrome, which are sometimes quite pronounced, but always with the same negative result. I had hoped to be able to detect something by the aid of *intermittent light*, and I used for this purpose, the bright flash of a powerful induction coil, condensed by Leyden jars. I found it somewhat difficult to manage, but it gave very sharp views of the ciliæ of rotifers, which, although they were in rapid motion, appeared to be still, but I could do nothing with the diatoms. The experiments however, were crude, and I have not the least doubt

that very fine and close lines can be seen with this mode of illumination more readily than by a continuous one, but the source of light should be very brilliant to give sufficient illumination. I have remarked that, as a general rule, the Nitzschiæ are the most active diatoms, though some of the Naviculæ e. g. *N. acuta* are not far behind them. And, though it seems to be necessary as a general rule, that the raphé, or the keel, should be in contact with some foreign substance, in order to produce motion, it is not always necessary. Thus, if one puts into a saucer, partly filled with water, some material containing diatoms, and exposes it for a few days to the light, and especially if protected from the dust by a sheet of clean glass, a thin pellicle or skin will be found all over the surface of the water, charged with diatoms, and later, a slime skin will be found on the bottom and sides of the vessel, also full of diatoms; and often, many species of different genera mixed. This "*thallus*," as it has been called, is a favorable "nidus" for many germs of the minute algæ, so that, along with the diatoms, one will find myriads of round, green or olive tinted spores, which have sometimes been supposed to be a phase of diatom growth. On damp walls, and where the water is dripping from an aqueduct, and around the overflow of basins of fountains, I have found these mucus skins in abundance, and in them, a great variety of forms associated; and not unfrequently, in quiet waters, these skins, stripped off from the stones or submerged plants by the buoyant power of the gasses eliminated in the sunshine from the diatoms and minute vegetable organisms, may be observed floating on the surface. A somewhat similar thin slime skin, appears to be left by some gasteropods, after moving about for some time, shell downwards, on the surface of the water. I made a curious experiment with *Physa heterostropha*, which after thus moving about awhile on the surface of the water, now began slowly to descend — so gently, and slowly, that it appeared to be suspended by an invisible thread attached by one end to the slime skin and

which it was now paying out, as it were, in its easy descent; with a pair scissors, carefully introduced into the water, I snipped this invisible thread, and instantly, the snail dropped like a shot to the bottom, I have often repeated this experiment which was performed in the first instance at the suggestion of one of my colleagues, and is really his discovery. Now, is there any such thing in the case of the diatoms? I I have not been able quite to convince myself of this, and yet, they often retrace their motions in a path very suspiciously near to that pursued before, as though guided by some Ariadne's thread, and I have often seen a diatom moving and tugging behind it, as if by such a thread, some small object, by a series of jerks, or twitches. I am sorry that these experiments, to determine if possible some cause for the movements of the diatoms are so meagre, and that I can offer so little towards a satisfactory explanation. It is nearly twenty years now since they were made, and we must confess that that our knowledge is still very imperfect, very little if any, in advance of what was known then.

IV. *Reproduction and Growth of the Diatoms.*

Here, as well as concerning the motions of the diatoms, there are many points still quite obscure, and much of what has been published, even recently, is based upon the statements made many years ago in the "Synopsis of the British Diatomaceæ," and which are decidedly untenable, and very far indeed from being proved. It was assumed by the Rev· W. Smith, in the work just named, that the different genera may be grouped, as to their reproduction, into four classes, and this assertion has been copied by nearly everyone who has written upon the subject. It is erroneous, inasmuch as we cannot select any one genus as a representative of this or that class; and as for the classes themselves, I am pretty certain that the fourth one which he mentions, has no existence whatever.

Conjugation, the act of reproduction, occurs in one of three modes.

(1) Two frustules unite their undifferentiated endochrome (true conjugation) — one a sperm cell, and one a germ cell, and thus, *one sporangium* is produced from *two parent* frustules.

(2) Two frustules, with differentiated endochrome in each, unite, and produce *two* sporangial frustules — two sperm cells, and two germ cells — that is, a sperm cell and a germ cell in each of the parent frustules, these reciprocally act upon each other, and the germ cell of one, fertilized by the sperm cell of the other, gives rise thus to *two sporangial cells* from *two parent* frustules.

(3) The differentiated internal contents of a single frustule reunite, producing *one sporangium* from *one parent* frustule.

It is difficult to see how this latter is accomplished, nothing like a sexual character has yet been observed, but after all, it is not unlike the same stalk bearing in the compass of the same flower, the ovule and the fertilizing pollen. How the Case IV. of W. Smith, as stated in the British Diatomaceæ, can occur, I am at a loss to concieve; he supposes a *single parent frustule* to produce *two sporangial frustules*, and cites in proof Achnanthes, and Rhabdonema. Now, I have witnessed the conjugation of *A. subsessilis* and of *A. exilis*, and in each case there were evident enough, two parent frustules. I can only account for the statement in the Synopsis by supposing that only a few specimens were observed, and these imperfect, and that one of the parent frustules was accidentally removed. As for Rhabdonema, the figure given in the British Diatomaceæ is by no means conclusive. In all the filamentous forms that I have observed conjugating, e. g., Melosira, and Orthosira (as figured also in the Synopsis), as also in Odontidium and Fragilaria, the case is that of a *single parent* frustule producing a single sporangium. In plate E. Fig. 1, of the Synopsis there is

shown plainly enough the two differentiated portions of one parent frustule passing out from the halves of the same within the enclosing gelatinous sac; and again, these are gathered into two more condensed portions in Fig. 2, and two sporangial frustules are apparently shown in Fig. 3 of the same plate though I think they are but the valves of one sporangium after self-division. Dr. Pfitzer, in the work I have already alluded to,* enumerates some sixty species, belonging to twenty-six genera, in which conjugation had been observed so far as he was informed up to 1871. Thirteen of these, belonging to nine genera, I have myself observed, and to his list I can add fifteen species, and four genera; they are named below, where the first column shows the class, and the last the date of observation.

Class.		
2	*Gomphonema dichotomum*,	Oct. 1861.
3	" *tenellum*,	Feb. '59, Mar. '60, Mar. '63, Feb. '64, Mar. '66, Feb. '68.
2	" *olivaceum*,	Mar. '60, Dec. '62, Nov. '64, Apr. '79.
2	" *ovatum*, n. sp.,	Feb. '59, Mar. '60.
2	*Cocconema parvum*,	Nov. '60, Oct. '62.
2	" *cymbiforme*,	Nov. '60, June '60, Feb. '63, Aug. '65, Dec. '65, Oct. '72.
3	*Cymbella cuspidata*,	Mar. '62.
2	" *Helvetica*,	June '60.
2	" *affinis*,	Oct. '62.
2	*Achnanthes subsessilis*,	Aug. '60.
3	" *exilis*,	Oct. '65.
2	*Cocconeis pediculus*,	Oct. '66, Oct. '69.
2	" *placentula*,	Oct. '69.
2	*Navicula amphirhynchus*,	Mar. '62, Mar. '66.
2	" *atomus*,	Dec. '59.
2	" *Viridis*,	Mar. '62.

* Ueb. Ban u. Entwickl. d. Bacillariaceen, Bonn 1871.

2 *Stauroneis gracilis,* Mar. '62.
3 *Nitzschia linearis,* Nov. '65.
1 *Suriella splendida,* Feb. '59, Oct. '59, July '60.
3 *Meridion circulare,*, Mar. and Apr. '68.
3 " *constrictum,* Mar. and Apr. '68.
3 *Fragilaria capucina,* Mar. '61, July '64.
3 *Odontidium hyemale,* Dec. '59.
3 *Orthosira punctata,* July and Aug. '65.
3 *Melosira Borrerii,* Apr. '62.
3 " *varians,* Feb. '59, Oct. '59, Sept. '65, Apr. '68.
3 " *nummuloides,* Apr. '62.
3 *Synedra ulna,* Oct. '66.

Thirteen of these belong to Class 3, one parent frustule producing one Sporangium; fourteen to Class 2, two parent frustules and a double sporangium; and only one to Class 1, where two parent frustules form one sporangium, and not a single instance of W. Smith's Class IV. It is not a little singular that the instance he gives of his Class I. (my Class 3), is *Himantidium pectinale,* which is contrary to all other experience with the filamentous forms, and indeed as conjugation occurs *in the filament,* where the *valves* of separate frustules lie opposed, while for conjugation, it is necessary for the *connecting zones* to be in this position, one is at a loss to understand how his fourth method of conjugation could be effected in this species, while it was yet in the filamentous condition. So far as the figure in the S. B. D. illustrating the conjugation of Himantidum is concerned, it shows no evidence at all of the two parent frustules, but rather of one whose detached valves can be seen at either end of the sporangium, and which is shown *double* in one of his figures; but here, as with the Rhabdonema, from self-division. In the majority of cases which come under Class 2, the parent frustules are imbedded in a mass of jelly, which, at this time appears to be secreted in great abundance; they

lie side by side for some days, the connecting membranes, or zones, in contact, not the valves; certainly three days in the case of *Gomphonema olivaceum*, and, during this time the internal substance swells, and the frustules open at the sutural line, and the two halves of the frustules are soon pushed so far apart, that the membranes enclosing the internal substance of the two frustules come in contact, and absorption taking place, the two endochromes are united, and segregation commences; two ball-like portions are formed, which are the rudiments of the future sporangial frustules, and as these are developed, they carry with them the valves of the parent frustules still adhering to the outer membrane which encloses the new individuals. Dr. Lanzi, of Rome, if I understand his meaning, seems to have made the mistake of a frustule containing germs, changing into a sporangial cell, and speaks of a series of tranformations, until the "Thallus," as he calls it, is changed into germs and frustules in various stages of development, which, if true, would be an entirely different mode of reproduction from anything I have ever observed. I am quite certain, from the many times I have seen *Gomphonema olivaceum*, and other diatoms conjugating, imbedded in mucus masses, which were largely a tangled mass of stipes, and charged with germs, not of diatoms, but of various algæ, that the "spores of diatoms," and supposed "diatoms in various phases of growth" seen by him, were not really such, nor indeed in any manner a phase of diatom growth, and I think I may say that there is no other mode of reproduction other than the formation of a sporangial frustule in the manner already described. This process as first noticed by Thwaites, and since by scores of others, is fully adequate for the purpose of perpetuating species, it is quite clear and simple, and in entire harmony with all the well-established phenomena of plant reproduction; but I shall have something more to say of "sporangial spores," and "brood diatoms," and their supposed development from minute germs, hereafter.

In the conjugation of some species, *e. g. Navicula viridis*, and *Stauroneis gracilis*, a rugose siliceous sheath is formed before the valves of the sporangial frustule make their appearance; this is generally cylindrical, with cap-like ends, and its rugosity appears to be due to its increasing along with the growing sporangium. This sheath has been by some considered as the sporangium, and the frustule formed within it has been improperly called the "Auxospore;" but it is not always present, and in fact is only found in a few genera. Representatives of this sheath may be seen in plate IV., and I shall allude to it again. Previous to conjugation, which occurs most frequently in the spring, the endochrome of the frustules preparing to conjugate, becomes paler, the colorless portion more abundant, and innumerable minute dark particles may be seen in incessant trembling motion, not unlike the granules in the little cavity at the ends of a lively Closterium; this phenomenon is particularly noticeable in the Gomphonemæ. I have tried in vain to discover some perceptible distinction or difference between the conjugating individuals. I fancied at one time that the little particles might be instrumental in effecting fertilization, but I have since observed them in great abundance in frustules where, evidently, the endochrome was already in a state of decomposition; of course I cannot positively assert that these latter were identical with those observed in the conjugating particles, they appeared to be the same. The amount of mucus, and its consistency, varies with different species; with the Gomphonemæ and the Naviculæ, there is considerable; with Stauroneis, very little. Again, it is very tough and strong with Surirella, and none at all with Melosira. These conditions may not obtain universally, and, doubtless, they are modified to suit circumstances.

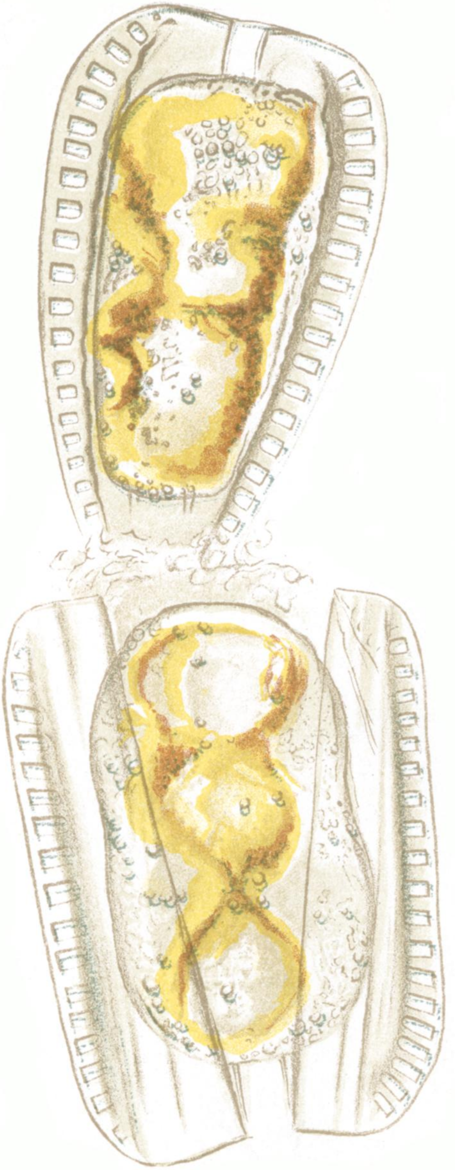
The time required for conjugation varies from six to eighteen days, and I give below the actually observed and measured growth of the sporangium of *Cocconema cymbiforme*, which I found conjugating Oct. 27, 1872.

Length of sporangial frustule at commencement,				.000833"
" " "	in	24	hours,	.000942"
" " "	in	36	"	.000978"
" " "	in	48	"	.001051"
" " "	in	60	"	.001123"
" " "	in	72	"	.001305"
" " "	in	84	"	.001522"
" " "	in	96	"	.001631"
" " "	in	108	"	.001885"
" " "	in	120	"	.002030"
" " "	in	132	"	.002175"
" " "	in	144	"	.002465"
Total 6 days: increase	first		day,	.000109"
"	second		"	.000109"
"	third		"	.000254"
"	fourth		"	.000326"
"	fifth		"	.000399"
"	sixth		"	.000435"

The rate of increase was much more rapid as the sporangium approached maturity. I kept these specimens, and numerous others to be mentioned in the record of measurements during conjugation, self-division, etc., living in an entirely healthy condition by means of the "growing slide," which I contrived for this purpose, and described in "Silliman's Journal" September, 1865.

The time observed for completion of conjugation of *Gomphonema olivaceum* observed in December, 1862, was nine days. The sporangium, when first formed and gathered into balls, measured .0005", and when fully grown .0018", and a sporangial frustule that did not exhibit a trace of division line at 9 A. M. exhibited at 11 A. M., next day, a very strong one. The Gomphonemæ generally, and the Naviculæ belong to Class 2. Two parent frustules and two sporangia, and frequently, as already noticed by Mr. Carter, there is a difference in the size of the former, but not always, and this is

PLATE II



probably a character of no significance. Among the Gomphonemæ there is one form which I consider as *G. tenellum*, that I found for several years conjugating, and only one parent frustule producing a single sporangium (Class 3). All others of this genus, so far as I am informed, belong to Class 2.

It will give a somewhat better idea of the conjugation of the diatoms to review particularly one or more instances belonging to each of these classes.

CLASS I. Two parent frustules and one sporangium, as in *Surirella splendida.* The conjugation of this fine diatom was, I believe, first noticed by Focke, but his specimens were not numerous enough, nor did he succeed in keeping them living long enough to arrive at accurate conclusions. His figure was copied in the Micrographic Dictionary. I found numerous examples of this diatom in the act of reproduction in October, 1859, and again in July, 1860. The two parent frustules unite at the smaller end, and in about two days, the sac containing the internal contents becomes so much swollen, as to open the frustule at the sutural line, and the vitalized enclosing membranes are thus brought into contact. I have represented this phase in Plate II. One of the parent frustules has already opened, and the enclosed sacs are well marked. As soon as the membraneous walls become obsolete at the place of contact, fusion of the internal contents begins and is completed in about six hours, at the end of which time the whole of the colored portion of the two individuals is gathered into a ball, and the colorless portion much resembles the white of an egg surrounding the yolk. This phase of growth I have represented in Plate III., and I have copied the appearance of the endochrome in both plates as faithfully as I could. I have never observed any further subdivisions and segregations of the yolk mass, or colored endochrome. At this early stage the commencement of the central band can be discerned, and up to this point, in the enclosing sacs (see Plate II.) and in the round

ball (Plate III.), the colorless portions were full of small globules, of not very much higher refractive power than the plasm in which they were embedded. I cannot here give a representation of the nearly completed sporangial frustule, which, if given on the same scale (magnified about 400 diameters), would be too large for one plate. It is about double the size of the parent frustules, and the swollen mucous envelope, seen around the ball in Plate II., is correspondingly enlarged, and still has the empty halves of the parent frustules adhering to it. The enclosing jelly-like or mucous sac, is not vitalized, it is simply an exuded substance, not entitled to be called the "sporangium" in the sense in which this name was given by Mr. Thwaites; but the continental authors have sometimes called it so, and have given the name "auxospore" to the sporangium of Thwaites. As the sporangial frustule of Surirella approaches full growth, the canaliculi begin to show well developed, the endochome is however pale, and a very large portion of the newly formed frustule is occupied by the colorless substance with the little balls. The final result of the conjugation is a large frustule, with much more rounded ends than the parent frustules, and valves not so wedge-shaped; it is, in fact, the *Surirella nobilis* of W. Smith, well shown in the figure given in the synopsis of the British Diatomaceæ. It exhibits very plainly the two nucleii of *Surirella splendida*, as described in Part I., as it should, being really one of the phases of growth of that diatom, and the specific name of W. Smith given to the sporangium must be deleted. I am not able to state the period required for completion of the conjugation, as I had not, at the time I found the diatom in this condition, invented the growing slide, and I could not, therefore, keep them living healthy and long enough to make as complete observations as I was able to do subsequently with other diatoms.

PLATE III

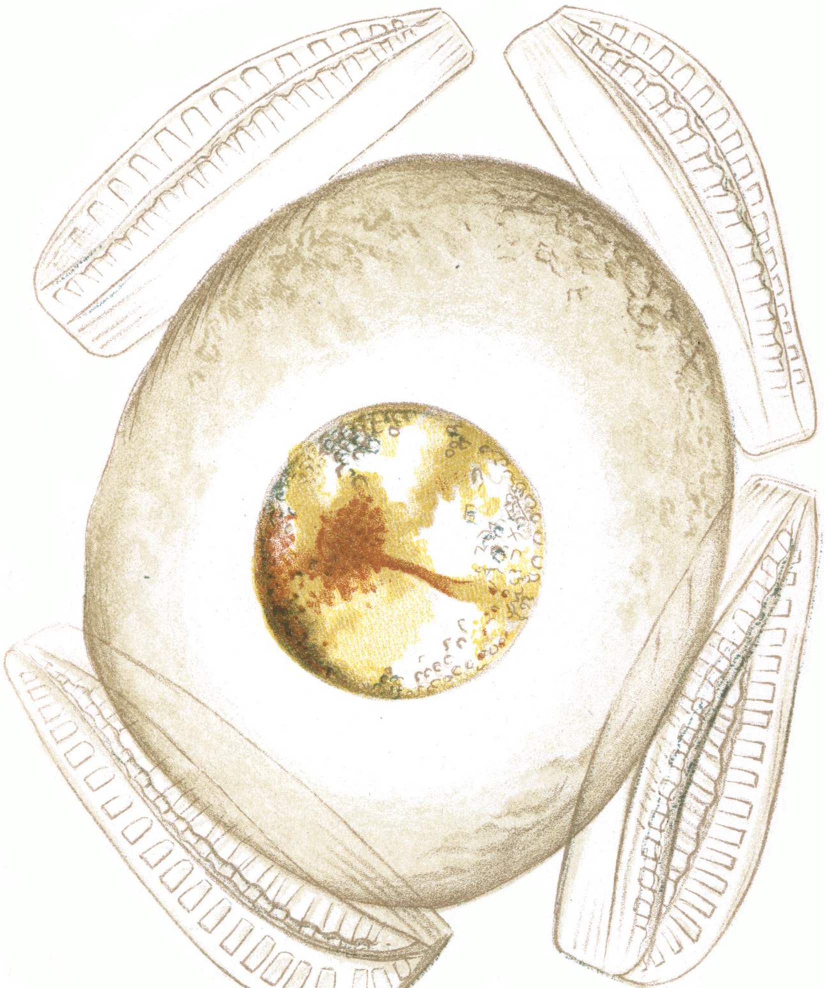


PLATE IV

Class II. Two parent frustules and two sporangial ones.

I select as the first example of this, *Navicula amphirhynchus* (*affinis*) which I obtained conjugating in March, 1862, and again in the same month, 1866. The parent frustules lie side by side Plate IV., Fig. 1, the endochrome soon becoming segregated into two well defined portions in each, and at this time, numerous minute dark particles, as already mentioned, may be observed trembling all over the endochrome sac in a very lively manner—these I have partly represented. Somewhat later, the double endochromes unite, forming two sporangial frustules (Fig. 2), which now grow side by side, and seen endwise, they appear as in Fig. 3. It would appear that the segregated portions (germinal and seminal?), though combined in the same individual and not in separate individuals as in Class I, act mutually upon each other, and so produce a double sporangium instead of the one just mentioned; and that in fact, it is a step removed in the scale of development from Class I. At a later date a rugose sheath is formedaround each sporangium, and at this time the jelly-like mass, that before enclosed the frustules, and which always has more or less of rubbish, grains of sand, &c., adhering outside, so that one has to focus through all this to get a view of the frustules themselves, disappears; and perhaps, this rugose sheath comes from the contraction and shrinking of this same silicious material. This phase of growth is represented in Fig. 4; the valves of the parent frustules may be seen still adherent, as the sporangial frustules move about. Fig. 5 shows the more matured sporangial frustule and rugose sheath in front view, and the completed form after the frustule slips out from the rugose sheath is shown in Figs. 6 and 7, in side and front view, and is what has been called *Navicula firma*. In some cases one of the sporangial frustules has proved to be abortive, but this did not appear to effect the growth of the other. I was able to make long continued and accurate observations of

this diatom, and to measure the growth of the sporangium from day to day. The average time required for the conjugation, was fourteen days, the length of the newly formed sporangium was .0018″ and when completed .00467″; the rate of growth was quite uniform. In the same gathering I found some of the sporangial forms conjugating, and so producing abnormally large forms, and I have witnessed the rupturing of the rugose sheath of one of these large individuals, and have seen the frustule escaping from it, leaving the sheath behind empty. I regret that, under the circumstances I cannot give the representations of *Navicula viridis*, and *Stauroneis gracilis* conjugating, these both, belong to Class II., and in both, I have observed, one sporangium to be abortive, and so apparently, if one should observe it near the completion, it might appear as coming under Class I. The rugose sheath of *N. viridis* is very firm, and it is not unfrequent in mounts of the prepared frustules of this species. *Stauroneis gracilis* is an interesting example, belonging to Class II. I obtained this diatom in the early spring of 1862, along with a mass of floating confervæ, from a small pond well warmed by the sun. The investing gelatinous sheath, was very tender; the slightest pressure on the cover-glass, would scatter the valves of the parent frustules, and the sporangium, often as a perfectly round ball, would roll about in the field. Unlike *Navicula amphirhynchus* and *N. viridis*, which, when even but half developed, are strongly enough impregnated with silex, to withstand strong heating, and of which I prepared many beautiful mounted specimens, those of *Stauroneis* are so tender, that I was unable to obtain any specimen which, after the burning, would show the rugose sheath. This sheath is however present, but it is much smoother than that of the diatoms just named. The globular shape of the young sporangium, is soon changed into an elongated barrel-shaped one, circular when seen in end view. Very soon after it becomes elongated, the endochrome shows a definite arrangement, and when viewed

PLATE V

endwise, a perceptible thickening of the walls may be perceived, corresponding to the enlarged central nodule of this species. The sheath has a cap on each end, and I have frequently seen the frustules swimming about with these caps still attached to their ends, while the central part of the sheath was gone. Mr. Carter has figured the same for *Nav. serians*, but I have never seen it break up into the thread-like phase which he describes, nor indeed can I see how this could happen. The time required for the conjugation of *Stauroneis gracilis* is from eighteen to twenty days, and the result, or final development of the sporangial frustule is *S. phoenicenteron* of W. Smith and well figured in the "British Diatomaceæ," this specicific name, however, must be deleted.

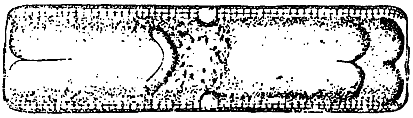
CLASS III. One parent frustule and a single sporangium.

This is the most common mode of conjugating with the diatoms, as well as the lowest in the scale; and, necessary with all true filamentous species. I select, however, for illustration here, a free form, *Cymbella cuspidata*, and the various phases are represented in Plate V. Fig. 1, shows the frustule imbedded in its gelatinous sac, just before and ready for, the act of reproduction, and Fig. 2, the same diatom, the endochrome differentiated, showing two well-defined globular masses. Fig. 3 shows the same somewhat more advanced, and as seen in side view, and fusion already commenced at the distinct clear space between them. A little later, by increase in the breadth of the internal mass, the frustule opens along the line of suture as shown in Fig. 4, in front, and Fig. 5, in side view, and now, the sporangium begins to increase in length, Figs. 6, 7, 8, and until the final result is the production of a completed frustule, and about double the size of the parent frustules, and shown in side view in Figs. 10, and 11. Here we have the internal contents differentiated, reunited, and the conjugative act all performed, with one individual. I consider it the lowest type. In the next higher, (Class 2) the differentiated substances of two individuals, become, not by reuniting in the same frustule, but by union

of the reciprocal portions of each, as they lie side by side, the origin of two sporangial frustules; and in the highest type (Class 1), the endochrome is not differentiated in either of the parent frustules, but they act simply as individuals, with a real, although otherwise not as yet discovered, sexual difference.

With regard to the office of the sporangial frustule, and the final development of parent frustules from these, it will be better understood after a consideration of the act of self division, which comes next in importance to conjugation, and I have been very fortunate in observing this. The time required for fissiparous division is much shorter than for conjugating, e. g. at 9^h 30^m P. M. I observed a small *Nav. viridis* with a strong division line, so that two individuals were clearly to be recognized; at 8^h 45^m A. M. next day a division line had commenced in one of them, and at 12 M. next day there was a well marked division line in the other, and at 8^h P. M., same day, not only were both divisions strongly made out, but the frustules themselves were now well rounded at their ends, and at 1^h 30^m P. M., on the third day, the main frustules, each with a strong division line in them slipped apart, and, as the division line in these was about the same as that of the original frustule when first observed, we may conclude that the whole time for complete division would be about six days. Another specimen, which for three days had been seen double, showed another division line between 3^h 45^m P. M. and 7^h P. M. in one of the halves. But it was with the large specimens of *Nav. viridis* (*Nav. major*) that I had the most complete study of the phenomena, and the accompanying wood cuts are from off-hand drawings made at the time, as the changes were too rapid to permit use of the camera; the colored endochrome is represented by the shaded portions.

At 10^h 25^m P. M. I noticed at each end of the frustule an infolding of the membrane enclosing the internal substance, and a fine wavy line on one side, Fig. (1). Five minutes



FIG, 1.

after I focused for the neucleus, which was very indistinctly visible, as it always is with this diatom, but the stellate arrangement around it was very marked. At this time, 10^h 30^m P. M. the division had progressed as shown in Fig. 2; it

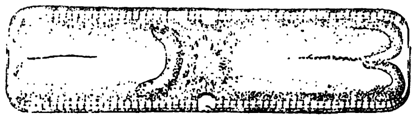


FIG. 2.

was deep within the internal substance, and could only be seen by carefully focusing, and the appearance was as though the membrane was infolding, funnel shaped, and I could distinctly see little particles running ahead of the line, and small globules now above, and now below the line. In three minutes more the division line was about three fourths the distance towards the centre, and at 10^h 36^m the line was completed as in Fig. 3.

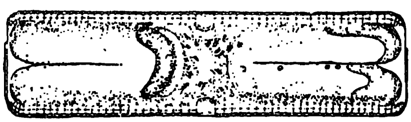


FIG. 3.

During this time slight changes, partly due no doubt to different focusing, appeared in the outline of the colored endochrome, but as the drawings were necessarily made in a hurry, I cannot pretend to any great accuracy in the representation of these.

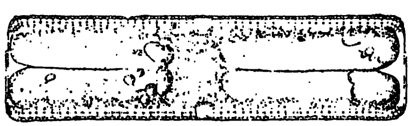


FIG. 4.

At 10:40 P. M. the appearance was as shown in Fig. 4 and now by carefully focusing on the membrane enclosing the colorless portions of the endochrome, I could see that this was covered with oblong granules, in a tremulous motion, reminding strongly of the so-called "willow leaves" *parva componere magnis*, to be seen on the sun. Small globules were also along the division line which was evidently thicker in some portions than others.

FIG. 5.

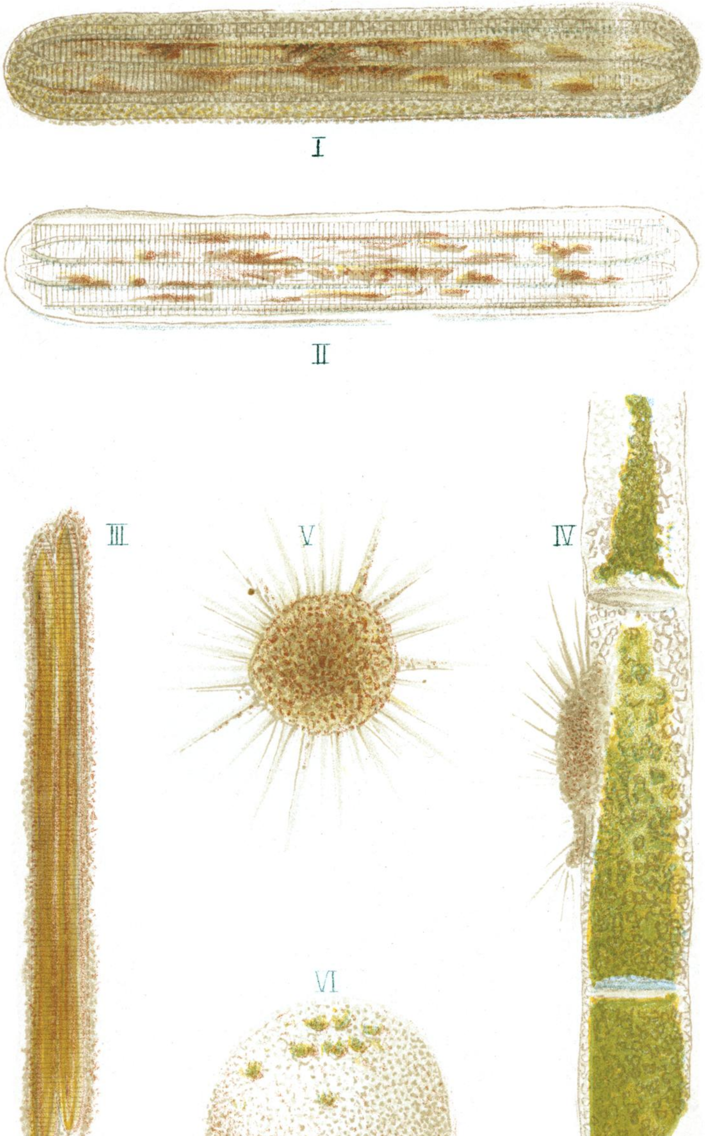
At 1^h 30^m P.M. the appearance was as in Fig. 5, a distinct white band crossing the central portion of the endochrome and dividing it, and the division line strongly marked; irregular large oil (?) globules or masses occupied the interior; these, in the course of a few hours, coalesced, and formed four balls in each half of the frustules, two against the central band and two at the ends. I did not observe the final self division completed to the separation of the two individuals of this particular specimen, as it was accidentally crushed, showing to me, however, two things which otherwise might have been unnoticed, viz., the membrane enclosing the endochrome was distinctly wrinkled where it had pressed against the inside of the old valves, but quite smooth at the new division; while the oil (?) globules, floating in the water outside the sac, behaved themselves like ordinary oils, giving a bright image of the light when the objective was drawn back from, and dark when advanced within, the focus. Many observations, made chiefly on the smaller forms, gave for the time for self division of this genus about six days, as already stated.

Much speculation has been indulged in as to the functions of the sporangial frustules. W. Smith and others have imagined that the contents were resolved into a brood of

young diatoms, and in the "Synopsis of the British Diatomaceæ" two figures are given, representing this supposed resolution; Dr. Pfitzer admits the same in the work already alluded to. I am quite disposed to admit that, as I shall show from my own observations made many years ago, as well as those of my valued friends, Mr. Kitton and the Rev. S. Lockwood, exceedingly minute forms of some diatoms, generally belonging to the group of Nitzschiæ, Naviculæ, or Amphoræ, exist, so minute even that they can, when living, pass through the pores of filter paper; but I have never yet seen any evidence that these can *grow*, i e., increase in size, for the process of self division is one of continued diminution, or, at least, not of increase. Indeed, I am myself quite certain that nothing like W. Smith's "broods of diatoms" has any existence; and, also, that the excessively minute diatoms, which I have seen as well as the skillful observers named above, never develop, by any growth of the frustules themselves, into the larger forms of Naviculæ or Nitzschiæ or other diatoms; but of this presently. So far as the "cysts" of W. Smith, enclosing "these broods of frustules," are concerned, I have found them frequently, and, enclosing in the same cyst, diatoms as widely differing as Gomphonema and Cocconema. It was upon a case of this kind that Dr. Bastian, in his book "Beginnings of Life," founded his supposed resolution of Euglena into diatoms, etc. The Synedræ, figured in the "British Diatomaceæ," Pl. B., Fig. 89, and the Cocconemæ, Pl. C., Figs. iii., iv., v. (221), are not, as he supposes, groups of young frustules resulting from a sporangium or from conjugation; they represent a phenomenon not uncommon, and are the work of a rhizopod (*Biomyxa vagans* (?) *Leidy*). I found these cysts of Synedræ in abundance, in October, 1858, often as many as six or eight of them in the field of view at once when using a $\frac{1}{4}''$ objective; and the number of diatoms included in the cysts varied from one or two, to fifteen or twenty, and occasionally, mixed with the Synedræ, were frustules of Gom-

phonemæ. The cysts, when completed and recent, were pale yellow, inclining to red, and full of granules. Plate VI., Fig. 1, shows the appearance of one of them enclosing a number of frustules of *Synedra capitata*, the color and arrangement of the endochrome of these show that they are already dead. The enclosing membrane of the cyst is rigid, and after about forty-eight hours, during which time the Amoeba (or Biomyxa?) has absorbed or assimilated the greater portion of the internal contents of the diatom, the membrane is ruptured, often at several places, and the plasm streams out in a long line through the opening, and then gathers up and begins moving around in search for something more that may serve for its development. It travels along the stems of the confervæ, as shown in Fig. 4, Pl. VI., or glides along the glass slide with a very different movement from the ordinary Amoeba, now sending out long reticulated pseudopodia, now contracting and changing in form; the granules are smaller and the color more pronounced, and the pseudopodia much finer than in the ordinary *Amoeba proteus*. Sometimes as they glide along the stems of the algæ the sarcode is drawn out into long thread-like portions, which are suddenly snapped, then again the mass gathers up; as soon as it reached a number of Synedræ, sessile on the algæ, instantly, and with greatest ease, it spreads itself all over them, as shown in Fig. 3, Pl. VI, and in the space of five minutes the whole assumed the appearance of a perfect cyst. In about two hours, as I repeatedly noticed, the frustules lost the beautiful arrangement of the endochrome of the living forms and assumed the appearance of dead ones; meanwhile the mass of the rhizopod was sensibly enlarged. It is easy enough to see why frustules, mainly of one species, e. g. Synedræ or Cocconemæ, should be included in a cyst, as these are sessile, or stipitate forms, densely aggregated, but if a Gomphonema or other diatom should be near by, that would also be drawn in. When, by rupture of the firm wall, the mass has escaped from the cyst, as I have described, the

PLATE VI



outline of the cyst is still distinctly visible, as shown in Fig. 2, Pl. VI, and the color and arrangement of the remaining endochrome in the included frustules is very much as shown in the representation in the "British Diatomaceæ," Pl. B, Fig. 89, a mere mass of red patches. The frustules themselves are, however, cemented together as if by partial solution, or deposition of cementing silex, and they cohere in bunches even after acid treatment and burning, but the enclosing cell wall of the cyst disappears. I have many specimens exhibiting this. Sometimes the motion of the rhizopod would continue for five or six hours, after which time, becoming quite globular, it would send out long radial threads, resembling somewhat *Actinophrys sol*, but without the peculiar pulsation vesicles, and frequently, when in this condition, little particles would run up and down the pseudopodia like beads on a string. This phase of growth is shown in Fig. 5, Pl. VI. One of these which I watched for over eight hours, became at the end of this time perfectly quiet, globular and without pseudo-podia, and much swoolen in size as in Fig. 6, Pl. VI; light green particles now appeared in it, somewhat like spores. After remaining in this condition for some time the ball burst suddenly, and the contents were scattered all over the field. I have seen hundreds of these cysts, and sometimes twenty or more in view at one time, enclosing frustules of *Colletonema vulgare*, and I am certain that they have nothing whatever to do with the reproduction of the diatoms, and that the formation of "broods of young frustules" from them is purely imaginary. It has been supposed that the contents of the Sporangium may break up and form masses of micro-gonidia, which in due time may become diatoms, and Rabenhorst has figured something like this; but the immediate and repeated self-division of the Sporangium and the production thereby of normal frustules is not consistent with this view. And moreover, so far as I am aware, it is supported only by the most doubtful observations. I have myself observed the granular

contents issuing from a broken frustule, somewhat as Rabenhorst indicates for Melosira, but I have never seen anything like the formation of diatom spores. Large zoôspores moving in a dead frustule are not uncommon, and I have numerous drawings representing them; and I have seen them inside frustules that were whole, so far as one could judge from rolling them over and over. These zoôspores are furnished with a long cilium, and their movements inside the frustule are lively. They were conspicuously present in a gathering of *Melosira varians* made in Feb. 1859. The filaments of this diatom showed many enlarged sporangial balls, and in some of the empty cells, frequently in an enlarged or sporangial one, were the zoôspores; and I have even noticed the granules, which for a long time had lain quiet in a dead frustule, to become lively and apparently to form into these spores, and I have also obtained them swimming freely in the water by rupturing the frustule. Whatever these zoôspores may be, I am convinced that they are no phase of diatom growth, they are probably a growth from germs in the water, minute enough to enter at the line of suture or perhaps along the raphè, and which have been developed at the expense of the diatom.

What then is the significance of the Sporangium, and especially an enlarged one? The true answer to this was first given by Braun in 1851, in the following words, "*The strange phenomenon that the primary generation, formed through the conjugation, attains about double the size of the parent cells, is simply explained by a *gradual decrease of size in the series of vegetative generations formed by division.*" The same explanation is given by McDonald, "Ann. and Mag., Nat. Hist." Vol. III, 1869 and by myself in the "Lens" in 1872. I had however, long before this, corresponded with Dr. Greville upon the subject, and found that he held the same views, and his letter bearing date Edinburg, Jan. 5th, 1866, and en-

* "Rejuvenescence in Nature." Henfrey's translation. Ray, Society, 1853; foot note, p. 132.

closing an illustrative pen and ink sketch, is before me now, and in it he names Prof. Balfour as entertaining the same views.

By the very structure of the diatom frustule, the valves, and the connecting membranes formed inside the old frustule by self-division, *must be smaller* than those of the original frustule, and Pfitzer in his work already alluded to, represents this on a somewhat exaggerated scale; and so it happens that in the course of time, by the continued acts of division of theSporangial frustule smaller and smaller individuals are formed, until at last the size of the original parent frustule, is reached, and at this juncture conjugation again occurs, and a new cycle is recommenced. No doubt myriads of frustules without ever conjugating, go on dividing, and so producing smaller and smaller individuals, theoretically without limit. Whether this be the origin of the minute forms often to be observed in great abundance in the skins coating vessels into which apparently only clean water has been put, is yet to be determined. Some experiments that I have made show that *living forms* which are very minute, may pass through a cloth filter that would arrest dead frustules of the same size, and especially may this happen under the action of light. Whatever may be the origin of minute diatoms appearing under conditions which seem to warrant a development from infinitesimal spores capable of passing even through filter paper, I am strongly persuaded that this explanation of them is a fallacy, and that the sole office of the Sporangium is the one indicated above and not the formation of spores of any kind.

The remarks which I have made as to a return to the normal size after repeated divisions of the Sporangial frustule are inapplicable to forms which do not produce Sporangia longer than the parent frustules, e. g. the Fragilariæ as a general rule, and other genera in which the connecting membrane of the one valve does not overlap that of the other, like the cover of a box, but which simply have the

edges of this membrane in contact and of the same size on each valve. The greater portion of the diatoms however, do have the *overlapping* cover, or "hoop" as some have called it, and I have repeatedly measured the difference in length of the two valves of the same diatom, e.g. a large *Navicula viridis* (*major*,) caused by the thickness of the overlapping "hoop" or zone. I found in the species just named, that the average length of the Sporangial frustule was .0072″, and of the parent frustule .0038″, and the difference in length between the old and new valves, in the first self-division was, .000125″; and therefore we may infer, that if the division was repeated some thirty times, we would have a return to the size of the parent frustules, if the difference in length continued the same between the old and new valves; probably this difference is less than half in the smallest frustules what it is in the largest, and instead of thirty, more likely sixty or more divisions occur before the frustule is reduced to the normal length, which, on an average of six days, already determined as period for division, would require a year! With our present knowledge however, this estimate can only be considered as a rude approximation, though there is much to warrant a belief, that for the larger diatoms at least, conjugation occurs normally once a year, and approximately, under the same climatic conditions. Among the diatoms the Gomphonemæ make their appearance the earliest. I have often obtained them conjugating from under the ice, and in early spring, every submerged leaf and twig in the running streams is covered with a mucous mass of tangled stipes, full of these diatoms, and forming tufts sometimes an inch in length.

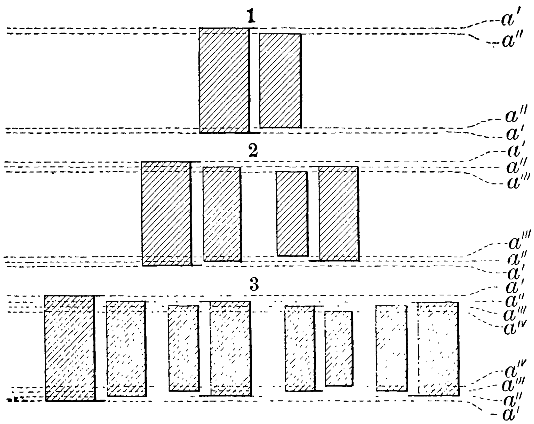
The astonishing increase in numbers by self-division is a matter of easy computation; thus, commencing with one frustule, after twenty-five divisions of the first individual continually repeated, there must be, supposing the new individuals also and their progeny to go on dividing during the interval, 33,554,432 individuals, and after fifty divisions

of the original frustule, no fewer than 1,125,899,906,842,624 individuals if all should live, but allowing say one-eighth of all the dividing frustules to be destroyed at each successive division, yet even then we would have from twenty-five divisions one million and four hundred thousand frustules nearly, from the original one! It is this rapid increase that makes the diatoms so abundant a few weeks after their first appearance, and in experimenting upon the raising of diatoms in the laboratory, the accidental introduction of one small form may in a few days or weeks give an abundant crop. While therefore I am far from positively asserting that the diatoms do not come from spores, and that they cannot grow i.e. increase in size, I am yet firm in my convictions that this does not occur, nor can I see any necessity for it. The function of the Sporangium appears to be sufficiently performed, in again renewing the cycle, precisely after the manner described, and to have it, instead of doing this, break up into a mass of spores of infinite minuteness, and develop forms by growth of a species which we know from actual observation does in reality become smaller instead of larger by the act of self-division seems to be a departure from the simplicity and harmony we find elsewhere manifested in the growth and development of these minute plants.

When by continued division, long after the normal size has been reached by the diminishing Sporangium, the diatoms become very minute, it is probable that not much, if any appreciable difference can be discerned after the vegetative act, owing to the extreme thinness of the connecting zones, and hence when we find an abundant crop of these minute forms in a "thallus" or layer on the walls of the containing vessel, or late in the summer or fall on the stones in the shallow water, there is not much difference in size, all minute. I have seen some specimens of Amphora, as minute as the smallest germs of the fresh water algæ, and the *Nav. Saugerii* is almost of this character, as also *Nav. atomus.* It is not impossible and indeed in the case of

Nav. atomus I am pretty certain that I have witnessed it, that these minute forms may conjugate, and that the Sporangium may again do the same. *Cocconema parvum* is a small diatom, and when found is generally aggregated in great quantities in more or less mucous material. I have seen this form conjugating, and the matured Sporangium could not be distinguished from *Cocconema cymbiforme:* the latter I also found conjugating in the same gathering as also often independently and producing a much larger Sporangium very much like *C. lanceolatum.* From some observations I have made upon the filamentous forms, I am induced to believe that as the diatom becomes older, the time required for division becomes longer, and finally ceases, with defective circulation and the frustule dies. In Rhabdonema and Striatella, and to a certain extent Melosira, there are some modifications. I have seen e. g. as many as twelve frustules of the latter in a filament with the halves of the original Sporangial frustules at either end, and yet, the frustules regularly diminishing to the middle of the filament, which could not have happened if they had continued to divide all at equal intervals, giving 2, 4, 8, or 16 as the numbers in the filament at any one time, besides, it is difficult to see how they could in this way decrease regularly to the middle. In Rhabdonema and Striatella, when the interior plates (pseudo-valves) are perforate, there is no difficulty in seeing how the two old valves can be pushed very far apart, with the perforate ones between, but no such structure exists in Melosira. In the ordinary way, supposing we have a frustule to commence with whose valves may be designated a^1, and a^2, the latter smaller and slipping out from a^1, we would have as a result of the first division, two frustules, whose valves would be, a^1 and a^2 slipping out from it for the first frustule, and a^2 with a^3 slipping out of it, for the second frustule; and the four valves of these two frustules in order would be a^1 a^2, a^3 a^2, *i. e.* the old valves a^1 a^2 would be the end valves of this filament of two frustules. Suppose now each of these two, to di-

vide simultaneously, we would have four frustules, the valves of the first frustule would be a^1 and a third a^2, slipping out from a^1; for the second a new a^3, slipping out from the second a^2 for one of its valves, and the second a^2 for the other; for the third frustule, the valves would be the first a^3, and a^4 slipping out of it; and for the fourth would be a third a^3 slipping out of the first a^2. Here as before, the old valves a^1 and a^2 would be the end ones of the four frustules in the filament, but two other valves equally as large as the original a^2 would also be in the filament, and the smallest valve of all viz. a^4, *would not be in the middle.* The diagram below will make this plain.



(1) Shows the original frustule with valves a^1 and a^2.

(2) After first division, having a^1 and a^2 for the ends, the new a^2 slipping out of the former, and a^3 slipping out of the original a^2.

(3) Result of second division, a^1 and a^2, original valves of (1) are still the end ones, but in the first frustule, a^1 has a third a^2 slipping out of it, and in the second frustule a^2 has a new a^3 slipping out of it; the third frustule has for its valves the first formed a^3 and the smallest one of all a^4 slipping out of of it; and the last frustule has a third a^3, and the original a^2 as its valves.

Pfitzer has illustrated this in his plate 6, in the work already referred to. It will appear plain now, that in Melosira, the Sporangial frustule divides, increases in breadth, again divides and so on, the frustules still cohering by the opposed valves, and doubtless some communication between them throughout the chain, though not infrequently this is interrupted; and, sometimes after the division line is formed, the development of one side is arrested, and the new valve, by the increase in breadth of the frustule, is pushed close against the inside of the old one, and this may happen over and over, so that several halves will be formed lying one inside of the other, like a series of empty beaker glasses, and this condition is notably true of *Orthosira Dickeii*, and is not uncommon in some Naviculae e.g. *N. clepsydra* and *N. rhomitica* of Gregory (I am inclined to think that the latter is but *Schiz. Grevillii.*)

The difference in size arising from continued self-division is easily to be noted in a filament of Melosia with a considerable number of frustules. From what has been said it will be understood that in a long chain there will be nodes, so to speak, not necessarily a regular decrease, and when a filament or chain is broken, very likely the smallest frustule may be left at one of the ends. In the chain of twelve frustules of *Melosira varians*, which I have before mentioned as developed from one Sporangial ball, there was a difference of 10 divisions, of the graduated head of a Powell & Leland spider line micrometer, equal to .00033″ between the diameter of the end and of the middle frustules. Although I have never observed any other mode of reproduction, and increase in numbers, than that I have described, it is proper to say, that many persons who have studied these little organisms, and whose opinions are not to be set aside lightly, seem satisfied there *is* some other mode necessary to explain the appearance of diatoms suddenly where none were before to be found, and where it seems they could not have been present except as minute, nearly or quite invisible germs. I have

already said I am not disposed to admit this, but my testimony is merely of a negative character, I cannot disprove it, and I shall, myself, add some testimony, which, at first appears to warrant this view: e.g. I found for many successive years the same diatom (a straight form of *Meridion constrictum*) growing in large filaments in rain pools, made at the same place in a meadow in the Spring, but which was dried up through the Summer and Autumn, and the ground baked hard by the heat of the sun. To explain this we may suppose that, as the dry season approached, the diatoms following the moisture retreated farther and farther into the the ground, and possibly remained there dormant in the absence of light, until the succeeding Spring; coming again to the pools formed at this time on the surface, and commencing again a new cycle by conjugation, a cycle for which their long retreat had prepared them. It has been too generally supposed that the diatoms are mere surface forms, and do not live at any considerable depth. I have taken them however in pretty deep water in a sounding cup, and by the dredge in Seneca lake and in the most flourishing condition, and from an examination of the soundings of the U. S. Steam Ship Tuscarora, I am pretty certain that great belts of "diatom ooze" or *living diatoms* exist at depths of from three to four miles, and in fact that at this depth, they are performing the same office in appropriating carbon and eliminating oxygen, as the land plants, and that, without their presence, many forms of marine life now known to flourish in deep waters, could not exist. The constant occurrence of *Gomphonema olivaceum* all over the northern part of the United States in the early Spring, and even under the ice in places where, later in Summer and Fall, hardly a frustule of it is to be found, seems to indicate something like a dormant condition into which they may pass, and from which they may awake again under the rejuvenating power of Spring, and I am disposed to believe, that, after the rapid Spring or early Summer increase, when conjugation has been affected, and to a certain

extent the vegetative growth has gone on in the Sporangial frustules, reducing them to their normal size, as the warm weather advances, and the waters no longer fill the brooks, and saturate the soil, they bury themselves, and remain more or less dormant until the time returns for a recommencement of the cycle; and as for the marine forms, they may practically, in varying zones of depth, do the same thing, but modified to suit the almost entirely different conditions as to heat and moisture from those experienced by fresh water forms. Anyway, it is an undoubted fact that marine diatoms are more abundant at certain seasons of the year, and that as far as littoral forms are concerned, and these embrace nearly all, they appear to be affected very much as are the fresh water ones. While then, as a general truth, we may accept it that the diatoms disappear largely at certain seasons, whether burying themselves in the moist earth or the soft mud or not, we still find that a great many do remain in the water at all seasons, or even exist on the surface of moist places, and these probably continue the vegetative growth indefinitely or until very minute frustules are produced. It has been stated, on what appears to be good authority, that diatoms which have been dried for a long time will again resume activity upon being moistened. My own experience does not confirm this, I have tried the experiment, but always with a negative result, and so, though I cannot assert the impossibility of doing this, I am firm in my own convictions, that a diatom cannot be dried and again revived. The living forms which have been observed when dried mosses have been subsequently wetted, were undoubtedly conveyed there in the water, and would not have appeared if distilled water had been used though they may with filtered water, inasmuch as minute living diatoms can pass when the dead frustule would be arrested, certainly they may pass quite as easily as spores. Their power of movement under stimulus of light may enable them to get through any pores wherever water can pass, and no doubt they do

this, and so it is not difficult to get a crop of diatoms from what appears to be pure spring water. That they are in abundance in the ordinary drinking water is sufficiently proven by tying a little bag of Canton flannel over the faucet of delivery, and allowing the water to run through for a while. In this way an abundant crop of diatoms and desmids may be obtained, and frequently other larger fresh water algæ; and a second bag, outside the first, will still show numerous, but minute forms passing through with the first filtered water.

As regards the vitality of the diatoms, I have had some curious experience. In August, 1862, I made a gathering of marine diatoms at New London, Conn.; they remained in the bottle and passed through the putrefactive fermentation, after which the water became perfectly clear and without the least odor; the gathering was mainly *Melosira Borrerii*. The latter part of October, myriads of minute Amphoræ were living, and hanging in festoons from the dead filaments of the Melosira; and in another gathering, made at the same time and place, I found, in July, 1864, two years after the gathering was made, multitudes of minute living Amphiproræ (*A. paludosa?*), and long filaments of Fragilariæ, the latter conjugating; and in May, 1865, in the same bottle, the Fragilariæ were quite vigorous and with some large frustules, and there were many lively Amphoræ of nearly the normal size. I examined it again in January, 1886, and found the same diatoms still living and healthy; but the June following they were evidently on the wane. A part of this time the bottle had been kept in the dark, shut up in a drawer, and once it had been frozen solid. Further observations on this identical gathering were prevented by my removal to Geneva, N. Y., and the bottle was either lost or destroyed. The experiment showed, however, that the diatoms can, some of them, withstand the putrefactive fermentation, which effectually destroys the larger algæ and animal life; and also that these minute forms produce larger ones, not by growth or increase in size, but by repeated acts of con-

jugation, as witnessed in the conjugation of *Nav. Amphirhynchus.* Among the bottles of the Tuscarora soundings there was one which had, apparently, only sea water in it; it was labeled "Specimen of sea water from a depth of 1,828 fathoms," or over two miles; the cast was made September 22, 1873. I exposed this to the light and heat of the sun for about a month, and I found in November, 1874, a thick yellowish olive deposit, from which minute bubbles of gas were given off, and examination under the microscope showed that it consisted of millions of minute Amphoræ. (*A. veneta?*). The bottle was now put aside and shut off from light until June, 1876. When taken out it showed only the colorless remains of the Amphoræ in the casual dips which I examined, but after exposure to the sun for a week there was a perceptible yellowish tint, and microscopal examination showed myriads of minute green spores, some of a decidedly yellowish tint and conspicuously like a double Amphora, of which there were now a considerable number living. April 5th, 1879, the round spores were still abundant, but not a single specimen could I find of a living Amphora, nor again in June, 1879. Another sounding, labelled "Sea water, 1,015 fathoms," in which I had a long time before found no living specimens, but to which subsequently I had transferred a small amount of the film from the other bottle, has shown, up to the present writing (1880), fine clusters of living Amphoræ and also some moving freely. In March, 1860, I made a gathering of *Gomphonema olivaceum*, and after two weeks' exposure in a saucer, covered with glass, found them conjugating. May 20th the Gomphonemæ were apparently all dead, but there were plenty of living *Amphipleura pellucida* that I had not previously noticed in the gathering. September 15th, the water unchanged, there were multitudes of Amphipleuræ, and also *Navicula acuta*, present and living. In October, and finally in January, 1861, there was a considerable growth of confervæ and a few living *Gomphonema tenellum.* These are a few of many examples

of which I have notes, but sufficient to show clearly that the diatoms may withstand the putrefactive decomposition, at least the smaller forms, and also freezing, and show themselves living after years. Only in the rarest cases have I found conjugation among these minute forms, and by this act an increase in size; generally they have remained very minute, and the growth was evidently a vegetative one. How long this can go on under proper circumstances it is impossible to answer now. Under the action of light it may be very rapid, but in darkness it is probably arrested, and precisely as I have arrested the development of a tadpole by keeping it almost excluded from light in a small aquarium for over six months, (how much longer, if it had not been accidentally destroyed, it would have remained in this condition I cannot say), so these minute forms may in the dark be dormant, until called into active life by proper conditions of light and temperature, when by rapid division they may become so numerous as to show their presence even to the unaided eye. So far as my own observations go, the Amphoræ, and the very minute Nitzschiæ and Naviculæ are the most tenacious of life. I will close this already prolix article with a description of one other experiment — a sort of vegetable photography. Crossing a shallow ford I was tempted to raise a leaf which, as well as the surrounding hard clayey bottom, was colored that peculiar olivaceous tint denoting a layer of diatoms. I raised the leaf, and a very pretty picture of it remained on the hard bottom; in other words, there appeared no diatoms under it. I scraped the surface of the mud, and filled a small bottle with the diatoms as pure as I could get them. Of course, in spite of all the care, there was enough of the clay-mud mixed with them to make the whole mass appear slate-colored when the bottle was shaken. On arrival home I spread some of this mixed mud and diatoms on a glass plate, as evenly as I could and about the thickness of a penny, and then laid upon it several thicknesses of moistened blotting paper; inverting the whole

and arranging so as to keep the paper moist, I exposed the glass side to the sun, having previously covered this with a bit of lace. In about an hour I obtained a tolerably distinct picture of the lace, the diatoms coming up to the surface wherever the glass was uncovered.

Desultory and incomplete as these observations are, and giving largely but negative results, yet the bare recital of them has called up most delightful memories. Once more I have been rambling along the Ohio streams and wild ravines, and once more I have seemed to inhale the brisk sea breezes and hear the cry of the startled sea birds. I have trodden again the solitudes of the great western lakes along an Indian trail, and then, so busy memory wills it, have been transferred to the charming water lanes of Guernsey. Once more I have clambered over the rocks at low tide and watched the sun go down behind theg reat Atlantic, at Colo Bay; once more collected diatoms after the deep descent to Moulin Huett, and here before me are the very gatherings made so many years ago. These came from Alderney where, cut off by the rapidly incoming tide, I climbed the nearly perpendicular cliffs; and these from Cherbourg,—classic ground for the diatom lover; and last, not least, these tell me of many long delicious summer days among the lakes and cold streams of Switzerland. But with all these are sadder, tenderer thoughts. And so if there be anything of value in what I have said in these papers about the diatoms, I offer it as a tribute to the memories of those now dead, who kindly aided and encouraged me,—Bailey, Gregory, Arnott, Greville, Eulenstein, De Brebisson, and others less known to fame, but not less dear.

EXPLANATION OF PLATE I.

Navicula major, in the indigo field; showing the currents produced along the valves; the formation of the little balls of indigo; and the investing mucous material.

EXPLANATION OF PLATE II.

Conjugation of *Surirella splendida*; the fusion is at the smaller end of the frustules, and one frustule has already opened by the swelling of the internal contents.

EXPLANATION OF PLATE III.

The sporangium of *Surirella splendida* after the complete fusion of endochrome; the valves of the parent frustules adhering to the mucous mass.

EXPLANATION OF PLATE IV.

Conjugation of *Navicula firma*.

FIG. 1.—Differentiation of the endochrome in the parent frustules.
FIG. 2.—Formation of the sporangia, with rugose sheath.
FIG. 3.—View of same endwise.
FIG. 4.—The sporangia fully developed in their rugose sheaths; the valves of the parent frustules attached.
FIG. 5.—A single completed sporangium.
FIGS. 6-7.—The completed frustule (sporangial) in side and front view, and after the rugose sheaths have disappeared.

EXPLANATION OF PLATE V.

Conjugation of *Cymbella cuspidata*.

FIG. 1—Parent frustule.
FIG. 2.—Differentiation of the endochrome in same.
FIG. 3.—Refusion of the differentiated portions.
FIGS. 4-8.—Phases of growth of the sporangium in its mucous envelope.
FIG. 9.—A completed sporangium with the valves of the parent frustules adherent.
FIGS. 10-11.—The completed (sporangial) frustule.

EXPLANATION OF PLATE VI.

FIG. 1.—Group of *Synedræ*, encysted by an amoeba.
FIG. 2.—Same after the escape of the sarcode of the amoeba, through the fissue in the wall of the cyst.
FIG. 3.—The encysting, before the formation of the firm boundry wall.
FIG. 4.—The amœa (Biomyxa,) travelling along a stem of conferva.
FIG. 5.—Same after assuming the ball form and showing the radiating pseudo-podia.
FIG. 6.—Same after several hours quiet, and showing formation of small green spores.